

As it is a difficult matter to purify completely the small volume of emanation and to keep it pure, the observed pressure of the emanation and mixed gases at the temperature of condensation was corrected for by taking the true volume of the emanation from 1 gram of radium in equilibrium as 0.585 cubic milligram. This calculated volume is in excellent agreement with the minimum value which I have found experimentally. As the emanation is apparently an inert gas of atomic weight 222, it is of interest to compare its boiling point with those of the heavier inert gases found in the atmosphere. The boiling points of argon, krypton, xenon, and emanation are, respectively, 86.9, 121.3, 163.9, and 208 degrees absolute. It will be noted that as the boiling point of krypton is about intermediate between that of argon and xenon, so the boiling point of xenon is nearly the mean between that of krypton and emanation.

If the capillary tube containing pure emanation is quickly placed in the pentane bath, cooled well below the temperature of initial condensation, under a microscope small drops of liquid emanation are seen on the walls of the capillary. The position of each globule is marked by a brilliant local phosphorescence of the glass of the capillary.

E. RUTHERFORD.

University, Manchester, February 13.

### Crocodiles and Tsetse-flies.

My attention has been directed to a paper read before the Royal Society of Arts by Mr. James Cantlie on January 27 called "The Part played by Vermin in the Spread of Disease," published in the society's journal (January 29, pp. 202-4). Mr. Cantlie is there reported to have said:—"In sleeping sickness the disease is transmitted by the tsetse-fly, and the crocodile is believed to be the alternative host, the fly serving as a carrier only" (p. 204).

I do not know upon what evidence or upon whose observations Mr. Cantlie based his statement concerning the crocodile, but to judge from many similar statements that have appeared from time to time recently in the Press, the idea seems to be generally prevalent that Prof. Koch either observed or believed that the crocodile was a "reservoir" host for the human trypanosome (*Trypanosoma gambiense*), just as big game is for the trypanosome causing the "nagana" disease of animals (*T. brucei*). Prof. Koch, however, has never expressed such a view in his published papers. In his last work on this subject, "Über meine Schlafkrankheits-Expedition" (Berlin: Dietrich Reimer, 1908), he wrote:—"Dem ersten . . . Krokodil entnahmen wir sofort frisches Blut, um Präparate zu machen und Kulturen anzulegen, und wir hatten in diesem Falle auch insofern Glück, als die Kulturen gelangen, wodurch wichtige wissenschaftliche Resultate erhalten wurden. Namentlich konnte auch festgestellt werden, dass das Blut des Krokodiles zwar Trypanosomen, aber nicht diejenigen der Schlafkrankheit enthält" (the italics are mine).

All that Prof. Koch showed was that the crocodile in the Victoria Nyanza is infected by a species of trypanosome, and that tsetse-flies (*Glossina palpalis*) feed on the blood of the crocodile. Both these facts had already been made known by English observers. The bare fact that the crocodile may be infected by trypanosomes is no evidence for connecting this reptile with sleeping sickness. The perch, bream, tench, and other fishes in the Norfolk Broads also commonly harbour trypanosomes in their blood, but are not to be regarded as a danger to mankind on that account. There is, in fact, no evidence whatever that the crocodile serves as an "alternative host" of the human trypanosome. It is inherently improbable that any reptile should play such a part.

I hold no brief for the crocodile, and should hear of its extirpation in the Victoria Nyanza without the least regret; I only desire that our scientific knowledge of the sleeping-sickness trypanosome should be correctly stated. It is possible, and indeed for many reasons probable, that a "reservoir" host for *T. gambiense* exists, but none has

been discovered as yet. Only the human species has been found so far to be naturally infected with the trypanosome of sleeping sickness, although many other mammals can be inoculated with it as a laboratory experiment.

Rovigno, February 10.

E. A. MINCHIN.

### The Production of Prolonged Apnoea in Man.

It is a matter of common knowledge that the time for which the breath can be held is increased by a preliminary bout of deep breathing, and divers often make use of this fact to increase the time for which they can remain under water. So far as I am aware, it is not usual to perform this forcible respiration for more than a short period, the pearl-divers of Ceylon, for instance, taking only a few deep breaths before descending; but in order to get the maximum effect a prolonged period is necessary. In my own case I found that whilst with no preliminary forced breathing I could hold my breath for only forty-two seconds, I could hold it for 2m. 21s. after one minute's forced breathing, for 3m. 21s. after three minutes' breathing, and for 4m. 5s. after six minutes' breathing (*cf. Journ. Physiol.*, vol. xxxviii.). The effect of the forced breathing is to wash out such considerable quantities of carbon dioxide from the blood and body tissues that even at the end of the three or four minutes' apnoea they contain less of the gas than when the breath is held for forty-two seconds without any preliminary forced breathing.

In theory, therefore, the deeper, more rapid, and more prolonged the forced respiration the greater its efficacy; but it is not so in practice. With some people the sensations produced by even a minute or two of forced breathing are very unpleasant. The hands and feet tingle and become numb, a dizziness is felt, and there is a strong disinclination to continue the breathing (*cf. Haldane and Poulton, Journ. Physiol.*, vol. xxxvii.). In my own case a period of eight minutes' breathing caused the muscles of the hands to pass into a condition of tonic rigidity, and they remained completely paralysed for the first 1½ minutes of the subsequent apnoea. Doubtless the unpleasant sensations are diminished by practice, but it is probable that for ordinary purposes it would be best not to continue the forced breathing for more than two or three minutes. Also there is a distinct element of risk if a diver remains under water almost to his limit after forced respiration. The amount of oxygen left in the lungs and blood then becomes so low that there is danger of fainting. Haldane and Poulton quote a case, of which they were informed by Dr. Collier, in which a diver lost consciousness when at the bottom of a swimming-bath after he had employed forced breathing to prolong his stay under water. Fortunately, he was rescued before death occurred, but undoubtedly the chance of fatality is increased by a preliminary forced respiration.

In the absence of forced breathing, the accumulation of carbon dioxide in the blood when the breath is held affords a natural safeguard, for it stimulates the respiratory centre to action with ever-increasing force, and ultimately compels respiration before the oxygen in the system has sunk to danger-level. However, the risk due to oxygen deficiency can be readily overcome. Hill and Flack have shown (*Journ. Physiol.*, vol. xxxvii.) that if a few breaths of oxygen are taken during quiet breathing, the time for which the breath can be held is generally more than doubled. Not only is the oxygen want of the system thereby eliminated, but, in addition, the oxygen renders the respiratory centre considerably less sensitive to carbon dioxide, and so permits it to accumulate to a greater extent than usual in the body. The same thing holds after forced breathing, and I found that if one to four breaths of oxygen were taken at the end of the forced respiration, the breath could be held about twice as long as in absence of oxygen. After one minute's forced breathing I held my breath for 4m. 18s.; after three minutes' breathing for 6m. 34s., and after six minutes' breathing for no less than 8m. 13s.

So far as I can ascertain, the world's record for a professional diver remaining under water in a tank was made by Miss E. Wallenda in 1898, when she reached 4m 45½s.

I do not know in what way divers prepare themselves for such feats, but presumably it is by a preliminary forced breathing only, without oxygen inhalation. Hence this record is probably comparable with my record of 4m. 5s., and in that case it follows that forced breathing, together with oxygen inhalation, might enable some individuals to stay under water for nine or ten minutes. Moreover, they could achieve such times without any risk of loss of consciousness. Even at the end of my eight minutes' record the air in my lungs still contained 46 per cent. of oxygen, or three times the normal amount.

The practical applications of this method of forced breathing and oxygen inhalation are obvious. Prof. Herdman states (Report of Ceylon Pearl Oyster Fisheries, part i., p. 63; part ii., p. 13) that the maximum time the best pearl-divers (the Arabs) remain under water is, in his experience, only ninety seconds, whilst the Tamil and other divers vary from thirty-five to fifty seconds. Of course, one would not for a moment expect them to attain the times above mentioned, as they are performing violent muscular work whereby the rate of production of carbon dioxide by the body is greatly increased. Still, there is little doubt that if they performed about two minutes' forced breathing, and took a single deep breath of oxygen at the end of it, they could, without risk, double or treble their average time under water. This might be of especial value to them when fishing in the deeper waters. Prof. Herdman says that while the usual limit of the divers is about nine fathoms, exceptional divers could go to fifteen fathoms, "but they had barely time to secure a single handful of the bottom before having to come up in an exhausted condition." The method might also be of value to sponge-divers, and to some extent also for rescue work in mines and drains poisoned by foul air, when proper rescue apparatus was not available.

22 Norham Road, Oxford.

H. M. VERNON.

### The Isothermal Layer of the Atmosphere.

THE difference of opinion between Mr. Hughes and myself apparently comes to this; he considers (February 11, p. 429) that radiation plays an important part in the temperature that is recorded by meteorographs sent up with a balloon, and I think that, save in exceptional circumstances, radiation may be neglected. We are agreed in stating that the temperature of the metal strip can only be altered by contact with the air and by radiation, and the only question is the relative values of these two causes. Furthermore, I gather that Mr. Hughes thinks that whether the ascent be by night or day, after a certain height the temperature is unduly raised by radiation from what he calls the hot planet.

Now, first, the thermograph is made of polished metal, and is protected by a polished metal case, and it is well known that a polished metal surface is not susceptible to radiation. One need only mention the double vacuum bottle in which liquid air is kept, the commercial "thermos flask." Loss or gain of heat by radiation is practically excluded by silvering the internal surfaces.

Secondly, it must surely be admitted that radiation must be very different by night from what it is by day. It is true that the sun subtends but a small solid angle, and the earth an angle of nearly  $2\pi$ , but the power of radiation varies as the fourth power of the absolute temperature. In saying that all radiation was insignificant compared with that of the sun, I was thinking of ordinary experience. In the tropics a man protects himself against the sun; to quote a very ancient writing, "there is nothing hid from the heat thereof." There are places in the high valleys of Switzerland where in calm, sunny weather a person may sit out of doors in the sun in perfect comfort, although the country round is deeply covered with snow and the temperature is far below the freezing point. On the other hand, in the Arctic and Antarctic winter it is protection from the wind that is sought; all accounts agree that if there be no wind extremely low temperatures are not unpleasant, and loss of heat by radiation is not feared.

But Mr. Hughes admits that radiation is stronger by day, and says that if it is not apparent on the trace it must be because the traces differ so much *inter se*. This is a question of fact, and I can only refer him to the pub-

lished records and to my previous statement that traces made in the day do not differ from those made at night. There is a striking similarity about the general form of all the traces, excepting those obtained in the daytime from a balloon which did not burst.

With regard to the vertical speed, we know that the time occupied in falling is about half that occupied in rising, because we have been able to ascertain by observations with a theodolite that the horizontal distance passed over during the fall is about half that passed over during the rise. We do not now use any parachute, and we used not to use one of more than 1 sq. foot area. The cross-section of the balloon before bursting is probably 25 sq. feet to 30 sq. feet. Inasmuch as at 20 km. height the air density is only one-sixteenth that at the ground-level, the initial rate of fall will be four times the final rate, and must therefore be greatly in excess of the rate of ascent. Notwithstanding this, the temperatures recorded are to all intents and purposes identical. Differences exceeding  $3^{\circ}$  C. between the up and down trace at the same height are very rare;  $6^{\circ}$  C. is the maximum recorded, and anything more than  $4^{\circ}$  is only produced by change of level of an inversion surface in the lower strata occurring during the ascent.

W. H. DINES.

### Barometric Oscillation.

IN NATURE of December 3, 1908 (p. 130), Mr. Dines, in reference to a previous note upon the semi-diurnal barometric oscillation, gave as his opinion that the semi-diurnal temperature oscillation is the result of pressure variation.

In connection with this question, it seems to me of some value to give here a couple of results derived from the Batavia observations. They are related to pressure oscillations of extra-terrestrial origin, like the semi-diurnal variation, and show a pressure change followed by a change of temperature.

At Batavia the well-known barometric oscillation with a period of  $3\frac{1}{2}$  years is very definite. It is followed by an equally distinct temperature oscillation of the same period. The difference in time is  $6\frac{1}{2}$  months. The temperature, moreover, shows the remarkable fact that the seven-yearly means from 1871-1905 are increasing regularly from  $0^{\circ}.08$  C. to  $0^{\circ}.10$  C. every seven years, so the temperature of the air increases by about  $0^{\circ}.01$  a year.

In the second place may be mentioned the influence of the 26-day period of the sun's rotation on meteorological data. A corresponding pressure oscillation is clearly shown. It is followed after nine days by a variation, of the same kind, of the temperature and the daily range of pressure.

There seems to be a close connection between the above-mentioned pressure oscillations and the influence of the sun's prominences on the earth. Both coincide in relation to time.

C. BRAAK.

Observatory, Batavia, January 11.

### Electrons and Atomic Weights.

LOTHAR MEYER suggested that the slight divergences between the theoretical and actual atomic weights in the periodic system might be due to the imprisonment of a quantity of the æther within matter; Lord Kelvin ascribed to the æther a weight of one-thousand billionth of a gram per cubic meter. Meyer's suggestion is hardly acceptable.

In the light of present-day theories of the perpetual disintegration of matter, it seems more likely that the atomic weights vary through loss of electrons; when the loss has reached a certain critical point a re-adjustment may take place, resulting in transmutation to a lower element.

If there be any truth in this theory, it may be supposed that the atomic weights of the elements may vary in different worlds of space, the more or less uniform weights found on the earth being due to the fact that the period of formation was identical in all cases. In this connection, it would be interesting to determine the atomic weights of the elements in meteorites, an investigation which I am unable to undertake at this time, but which I hereby suggest.

ALFRED SANG.

96 Boulevard de Versailles, St. Cloud, S. et O.,  
January 12.